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DR. M. J. M. HILL, F.R.S., professor of mathematics in the University College, has been elected vice-chancellor of the University of London for 1909-10.

DR. WALTER MURRAY, of the University of Dalhousie, Halifax, has been elected president of the new University of Saskatchewan, established at Saskatoon.

DR. ALLEN J. SMITH, professor of pathology, has been appointed dean of the medical department of the University of Pennsylvania, to succeed Dr. Charles H. Frazer.

THE *Journal* of the American Medical Association states that Dr. H. McE. Knowler, of the anatomical department of the Johns Hopkins University, has accepted a call to the University of Toronto, and Dr. Robert Retzer, of the same department, a call to the University of Minnesota.

DR. JOHN C. SHEDD has accepted the chair of physics in Olivet College.

DR. IRVING KING, who has been assistant professor in education at the University of Michigan for the past two years, has been called to the department of education in the State University of Iowa.

MESSRS. W. F. STEVE and PAUL DIKE have been appointed instructors in physics, and Messrs. Rufus A. Barnes and James Curry have been appointed instructors in chemistry in the University of Wisconsin.

THE following promotions and appointments have been made at Northwestern University: Dr. David Raymond Curtiss has been advanced from an associate professorship in mathematics to a full professorship. Dr. Robert R. Tatnall from associate professor of physics to professor of physics; Robert E. Wilson from instructor in mathematics to assistant professor in mathematics; Dr. Eugene H. Harper from instructor in zoology to assistant professor of zoology; Dr. James Caddell Morehead from instructor in mathematics to assistant professor of mathematics; Dr. Robert H. Gault has been appointed instructor in psychology; Dr. Charles S. Mead instructor in zoology, and Dr. Leigh Hunt Pennington instructor in botany.

DR. THOMAS H. BRYCE, lecturer in anatomy in the University of Glasgow, has been appointed to be regius professor of anatomy in succession to Professor John Cleland.

DR. JOHN MARNOCH, lecturer on clinical surgery at the Aberdeen Royal Infirmary, has been appointed regius professor of surgery in the University of Aberdeen in succession to Professor Alexander Ogston.

M. BOREL has been appointed professor of the theory of functions at the University of Paris.

DISCUSSION AND CORRESPONDENCE

GENERA WITHOUT SPECIES

IN his communication on this subject published recently in *SCIENCE*,¹ Mr. Caudell renders it clear that my reference² to certain correspondents cited by Professor Cockerell in a previous issue of *SCIENCE*³ as being either ignorant or inexperienced in some of the more difficult questions in nomenclature was not without warrant, at least in the case of one of the persons mentioned by Professor Cockerell. Inasmuch as Mr. Caudell, in his reply to my communication, has misrepresented (apparently unconsciously) my position in the case, I beg space for a few words more on the general subject of genera without species and other matters incidental thereto.

The logical inference from the general tenor of his article is that I am opposed to the International Code of Nomenclature, and would allow personal opinion to intervene in opposition to its rulings. On the contrary, I have been not only loyal to the International Code in all its bearings but have, in various papers published during the last two years, strenuously advocated its acceptance as *the* definitive code, in so far as its rulings meet the cases that are constantly arising in zoological nomenclature. Furthermore, where cases arise that are not clearly covered by the code I have urged that such cases be referred to the Nomenclature Committee of the International

¹ Vol. XXX., pp. 210, 211, August 13, 1909.

² *SCIENCE*, Vol. XXIX., pp. 934-936, June 11, 1909.

³ Vol. XXIX., pp. 813, 814, May 21, 1909.

Zoological Congress for arbitration, and that its decision be accepted as final. Still further, I have already submitted a number of such questions to this committee for decision, and stand ready to accept its decision of them, even should it chance to be adverse to my own personal views in the matter. This should answer Mr. Caudell's assumption, or at least insinuation, that I "hold that personal judgment should enter into the solving of this important problem" of genera without species, and that I am committed to "methods where personal opinion is given full sway." The tendency shown in frequent articles in *SCIENCE* and in various other scientific journals⁴ to refer difficult questions in zoological nomenclature to a committee of arbitration, whose decision, right or wrong, shall be final, I consider one of the most hopeful signs for the future in the nomenclatural field.

To come now to the particular question under discussion, namely, genera without species. In my former paper on this subject I claimed that each so-called speciesless genus should be considered by itself, on its own merits. As said before, it was considered the correct thing, a century ago, for a systematist to publish a synopsis of a class of animals, giving merely diagnoses of the generic and higher groups; at least many such synopses were published, and were then held in favorable estimation. Most of the genera in such cases had been already established by previous authors and stand, of course, on the basis furnished them by their founders, and had originally one or more species referred to them, but of course were without designated types. In these systematic synopses some new genera were proposed, which, if not homonyms, and were not given preoccupied names, have been accepted and long since became part of the established nomenclature of systematic zoology. There were not, however, full-fledged and properly habilitated genera, from the modern view-point, until the necessity for geno-

types became recognized and types for them had been duly designated.

Apparently Mr. Caudell does not see anything very absurd in recognizing an ornithological genus based on an unmentioned three-toed woodpecker, but thinks the case would be quite different with a genus based on an unmentioned species of hymenopterous or dipterous insect with a particular kind of forking of a wing-vein. I agree with him perfectly on both these points, for in the one case the species on which the genus was based is identifiable and in the other it is not. I am perfectly well aware that there are hundreds of speciesless genera that are absolutely unidentifiable, and that they are especially the bane of entomology. In every instance they should be rejected; but they can not be wholly ignored, since, as they are not *nomina nuda*, the name given them is preoccupied for further use in zoology.

The whole question of genera without species is badly muddled by bringing into it irrelevant matters. It is not difficult to decide what named groups are entitled, from the standpoint of the author who proposed them, to be regarded as "genera" (and in this connection subgenera must come into the same category), or have been recognized as genera in literature. The only point is whether they are good genera or bad genera—in other words, whether they are identifiable or unidentifiable from the basis furnished by the original founder. Of course there may be differences of opinion as to whether or not a certain genus is identifiable; but this is a question of zoology and not of nomenclature, although the result of any decision on the point will necessarily affect nomenclature. The simile of "a family of Smiths without a John or a Jane in it," or "a name Johnson before any one was born to bear it," is, to my mind, wholly beside the case; as is also Mr. Caudell's assumption that "a genus without a species has no object; it is a name applied to a conception, not to an object, and can therefore have no place in systematic nomenclature." This, it strikes me, is *reductio ad absurdum*. Identifiable genera without spe-

⁴ See especially Dr. W. H. Dall's "A Nomenclatural Court?" *SCIENCE*, Vol. XXX., pp. 147-149, July 30, 1909; and Dr. F. A. Bather, in *Ann. and Mag. Nat. Hist.* (8), Vol. IV., p. 41, July, 1909.

cies are based on previously known species whose characters are, in part at least, recognizably expressed in the diagnosis of the genus. When they are not, such genera have no basis and must necessarily be considered as non-existent.

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THE HYPOTHESIS OF "PRESENCE AND ABSENCE"
IN MENDELIAN INHERITANCE

IN our last report we gave reasons for regarding the rose-comb as a comb on which an additional element "roseness" has been superposed, and we suggested that the allelomorphic pair consists in the two states: presence of the factor for rose (R) and absence of that factor (r). The rose-comb is in reality a single comb modified by the presence of a "rose" factor. The same considerations apply to the pea-comb, which is single comb plus a pea-factor.¹

There are reasons for regarding man as a chimpanzee on which an additional element, "manness," has been superposed. There you have man expressed or explained² in terms of his anthropoid ancestor. The characters of a frog are undoubtedly latent in the frog's tadpole. What is to hinder, therefore, expressing or explaining the frog in terms of the tadpole by saying the tadpole carries the characters of the frog? The logic is sound in the statement that the tadpole contains "frog factors" or "frogness." The question is merely as to the helpfulness of sound logic used that way.

This seems like the method of reasoning that, as somewhere remarked by Professor William James, would enable Hegel and his followers to successfully support the hypothesis that men are always naked—under their clothes.

I am not ailing with metaphysico-phobia. Quite the contrary: upon occasion I enjoy

¹ "Experimental Studies in the Physiology of Heredity," by W. Bateson, Miss Saunders and R. C. Punnett in "Reports to the Evolution Committee of the Royal Society," Report IV., 1908.

² A few scholastics, more Abelard-like than the generality in keenness of dialectic, point out that there is an important distinction between "expressing" and "explaining" modern phenomena such as these.

and can profit by a half-holiday in some cool, shady dell of the land of metaphysics. I recognize, nevertheless, that as a rule it is a misfortune for metaphysics to get mixed with objective science. I recognize further that however unfortunate the mixture may be at its worst when deliberately made, by far the most unfortunate is such a mixture when made all unconsciously on the part of the mixers.

The opening sentence of Huxley's essay "Scientific and Pseudo-scientific Realism" is this:

Next to undue precipitation in anticipating the results of pending investigations, the intellectual sin which is commonest and most hurtful to those who devote themselves to the increase of knowledge is the omission to profit by the experience of their predecessors recorded in the history of science and philosophy.

Were the distinguished fellow of the Royal Society who wrote these lines living now, and were he a member of that society's evolution committee, he would, suiting action to word, almost certainly have saved his fellow committeemen the labor of discovering that the "allelomorphic pair consists in the two states, presence of the factor for rose (R) and absence of that factor (r)," by referring them to Hegel's "Logic," wherein the "divine principle" of *Negativität* is so fully and clearly set forth that its applicability to such cases as this becomes unmistakable.

Difference implicit or in itself is a difference of the essence, and includes both the *positive* and the *negative*, and in this way: The positive is in the identical connection with self in such a way as not to be the negative, and the negative is the difference by itself so as not to be the positive. Thus either is on its own account, in proportion as it is not the other.³

Again:

The foundation of all determinateness is negation (as Spinoza says, *Omnis determinatio est negatio*). Opinion, with its usual want of thought, believes that specific things are positive throughout, and retains them fast under the form of being. Mere being however is not the end of

³ "The Doctrine of Essence," in "The Logic of Hegel," translated by William Wallace.